

The wave-lengths of the bands, and other positions in the spectrum, roughly obtained, by which it may be possible to identify some of them in photographic spectra, although open to some uncertainty from the inconstant length and strength of the arc of flame in the electric lamp, which confused and shifted some of the comparison lines, were as follows:—

Electric Arc with Carbon Poles.	Wave-lengths.
End of the violet field ($\frac{1}{2}$, K β , and last violet line in arc-spectrum of strontium, 4,080–4,100)	About 4,100.
First light-band; faint violet-grey (H $_1$, 3,968; H $_2$, 3,933)	About 4,000–3,950.
Second do., strong grey band	{ About from 3,900 to 3,800.
(Strong grey line of mercury	{ About 3,700.
Third do., faint, grey	{ Between 3,600 and 3,500 (?).

Other metallic arc-spectra probably present lines in this portion of the spectrum, of which it would be interesting to examine the apparent brightness and the colours. At present the most conspicuous that I have met with is the grey line of mercury, which is brighter and more refrangible than the grey band of the electric light between carbon points. Its very advanced position in the spectrum, and the absence, or negative appearance of colour in its pretty bright light, both taken together seem to indicate very clearly that the grey or "lavender-grey" division of the spectrum fully equals in extent, when seen prismatically, the violet, the indigo, the blue, or any of its other better known, and much more ordinarily visible companion regions, the seven Newtonian colour-spaces of the spectrum. A. S. HERSCHEL
College of Science, Newcastle-on-Tyne, April 26

Pele's Hair

I HAVE read with great interest Mr. Moseley's description of Pele's Hair in NATURE (vol. xv., p. 547), since it furnishes information which I was most anxious to obtain. It seemed to me extremely probable that the analogy between Pele's Hair and the artificial furnace products would not be confined to the long fibres, and I did my best to ascertain whether irregular glassy spherules occurred along with the natural products. I was unable to obtain specimens for examination, but paid a visit to my friend Mr. J. G. Sawkins, F.G.S., who had explored the crater and collected the hair, in order to ask him whether he had ever noticed the pear-shaped spherules. He told me that he had never seen anything but the glassy fibres. I must say that I felt very much inclined to believe that the specimens usually collected are the material which has been blown some distance by the wind, consisting of the fibres from which most of the spherules have been broken. Mr. Moseley's letter in NATURE, and another which he has kindly addressed to me, make me believe that the analogy between the artificial and natural products is more complete than I was able to ascertain before Mr. Moseley's observations were published. In conclusion I would say that these facts in no way invalidate my arguments in respect to meteorites. They merely show that in certain cases the glassy volcanic spray, like melted furnace-slag, can to some extent collect into more or less imperfect spherules, so far analogous to those in meteorites as to indicate how those remarkable bodies were formed, but these spherules are accompanied by many fibres, which I have never yet seen in meteorites. This difference appears manifestly to depend on the difference in the temperature of the space into which the glassy spray was thrown. If the temperature of the air in the crater of Kilauea were equal to that of the melting point of the lava, we should almost certainly find, as in meteorites, many spherules and no hairs. H. C. SORBY

The Critical Point of Carbonic Anhydride

As the writer is not aware that any attempts have hitherto been made by others to exhibit to a large class the phenomena attending the passage through the critical point of a liquid in the presence of its gas, he is of opinion that the following account of a method which he has found very successful may be of interest:—

Dr. Andrews's apparatus for the study of gases was employed in the experiments, and the image of the tube containing the carbonic anhydride was projected on a screen by means of the oxy-hydrogen lime-light and a solar microscope which magnified

it about 120 diameters. Dr. Andrews's apparatus consists of a thermometer tube filled with carbonic anhydride and a second tube filled with dry air, which serves to measure the pressure applied. The lower ends of these tubes dip beneath the surfaces of mercury contained in test-tubes, which are suspended in strong copper cylinders communicating with each other, and filled with water, which presses on the mercury in the test-tubes. The pressure is applied by means of long steel screws which pass through the bottoms of the cylinders. For the filling and mounting of these tubes the University of Cambridge is indebted to the kindness of Dr. Andrews. The lantern was supported on three screws, which allowed it to be raised or lowered so as to bring any required portion of the thermometer tube into the field of view of the microscope. The best height for the lantern was found to be such that the top of the tube was rather less than half an inch above the axis of the microscope. When the oxygen was turned on, the radiation from the lime cylinder raised the temperature of the portion of the tube within the field of view above the critical point in little more than a minute, so that no other source of heat was required; but when the oxygen was turned off the tube cooled through several degrees.

The best method of performing the experiment is as follows:—The lantern having been properly adjusted, the gas should be lighted, the oxygen turned on, pressure applied until the surface of the mercury comes into the field of view and the microscope focussed so as to give a distinct image of this surface. The pressure should then be relieved and a blast of cold air from a bellows or gas bag directed against the tube. This will cool it considerably below the critical point. The pressure should then be increased, the cold blast being continued until the inverted image of the concave surface of the liquid reaches the middle of the field of view appearing as a broad dark line possessing considerable curvature, and, of course, concave downwards. The focussing screw should now be finally adjusted so as to give the best image of this surface, and the blast then stopped. Immediately after cutting off the blast the operator must obtain command over one of the screws and carefully increase the pressure as the temperature rises so as to keep the image of the liquid surface just above the centre of the picture on the screen. As the temperature and pressure increase the broad image of the surface becomes narrower and less concave until, as the temperature approaches the critical point, the line becomes very thin and faint and loses its curvature altogether; it then seems to explode into mist and vanish as the critical point is reached. Another half turn of the screw then produces the well-known clouds or flickerings, which are best seen on the screen somewhat below the middle of the field, and in a few more seconds all is steady. More pressure should then be applied until the mercury reaches the axis of the microscope, but no change of state will be manifested by the carbonic anhydride.

It is important that the image of the surface of the liquid should not be below the centre of the field of view on the screen, for if the liquid stand in the tube above the axis of the microscope, since the greatest heat is there concentrated, bubbles of gas are liable to be formed within the liquid and to damage the continuity of the surface. Perhaps the flickerings may be due to unequal temperatures at different parts of the tube, so that some are just above and others just below the critical point. The mode of propagation of a sound wave through a substance just at the critical point may be an interesting subject for inquiry.

After passing the critical point the blast of air should be directed against the tube for about a minute. This will, of course, cause the image of the mercury to descend upon the screen, but no change of state will appear to take place in the carbonic anhydride. The pressure should then be rapidly diminished by turning the screws, when a violent ebullition will be seen, showing that the whole of the contents of the tube had assumed the liquid state during the cooling, the gas having passed at the critical point into the liquid without breach of continuity, so that no indication of a change of state was apparent on the screen. On increasing the pressure and continuing the blast the liquid surface will again appear, and the experiment can be at once repeated. WM. GARNETT

Camden Laboratory, Cambridge

Floating Cast Iron

HAVING read the interesting letter on this subject which appeared in NATURE (vol. xv., p. 529), I send the following copy of notes of experiments which I made about three years ago.

Several pieces of pig iron were put into a ladle (holding about one ton of metal); these at first sank, and a rush of hot metal took place upwards; after a few seconds the pieces of pig iron appeared floating, with very little of their bulk above the surface of the molten metal. A piece of flattish metal of irregular shape floated with a small portion alone of its corners above the surface; it was close to side of ladle. Pieces of flat cast-iron bars, $20'' \times 2'' \times 1''$, were carefully placed on surface (the latter being well skimmed); they floated without going below the surface. One of these pieces, which was put in *end on*, kept in this position for a few seconds, with its upper end above the surface; the other end then came up and floated on its flat side. In some cases a sharp crack was heard when the metals touched, and a white flame on one occasion burned like a gas jet from the side of one of the pieces.

The surface of the molten metal was in constant motion due to the currents within its mass, and showed the variegated texture or "break" peculiar to this condition of the metal. From notes of an experiment which I arranged for, but did not see carried out, I find that a cast-iron ball of about $2\frac{3}{4}''$ diameter, when lowered by a fine wire upon a well-skimmed surface of molten cast iron, disappeared completely at first, and then in a few seconds rose and floated with about half an inch diameter of surface exposed; it was then raised from the metal, when it showed a red glow on the lower part. It was again lowered, but now did not sink, but floated with about twice the surface exposed, as on the first experiment.

Different views are held as to the behaviour of cast iron when passing from the molten to the hot solid state, and finally to the cold (or ordinary temperature) state.

Some hold that the molten metal, on solidifying, expands like water passing into ice, and that it retains this expansion to such an extent that the cold solid is specifically lighter than the molten metal. Others hold that no such expansion takes place, and that finally the cold solid is specifically heavier than the molten metal. A third view is that the molten metal on solidifying expands, and that it then contracts during cooling, until it reaches ordinary temperature, when through the cooling it is specifically heavier than in the molten state.

From the fact that in foundry practice the linear contraction is taken at $\frac{1}{16}$ th part, there can be little doubt that the finally cooled solid is specifically heavier than the molten metal; again, from the sharpness of form of iron castings and other circumstances, expansion appears to take place on solidification.

The above experiments, I think, favour this latter view, as the floating took place more readily with small than with large pieces, partly due to their relative bulks and surfaces.

A probable explanation, in part at least of these phenomena, I think, is that the cold metal, when at first put in, is specifically heavier than the molten metal, but owing to the great heat around it (over $2,000^{\circ}$ F.) it is rapidly heated, and consequently expanded, and when sufficient volume has thus been obtained it floats. It is evident that small pieces, being more readily heated, may remain floating, whilst heavy pieces, whose volumes are larger in proportion to their surfaces, will take longer to heat, so as to induce the required change of volume, and may therefore at first sink, remaining below the surface till sufficiently expanded to rise and float. The experiment with the ball bears out this well, as, being a sphere, its surface was a minimum.

These experiments appear to corroborate very well those of your correspondent.

The following experiments which I lately made with lead may be of interest:—

An ingot of lead of 14 lbs. weight was placed on the surface of about 160 lbs. of molten lead; it at once melted. After allowing the metal to cool a little, an ingot was carefully placed on the surface, when it immediately sank, bubbles rising up to the surface; it was heard to strike the bottom of the ladle. Another ingot was tried; it also sank, and could be felt at the bottom (these ingots were cast from the lead in the pot). A small solid piece was cast of about $1\frac{1}{2}$ lb. weight, which also sank. Pieces of sheet lead were rolled up and placed on surface; these floated: the contained air and great surface in the latter would account for this.

These latter experiments with lead correspond very well with those of your correspondent with zinc. W. J. MILLAR

Glasgow

Yellow Crocuses

In my garden the sparrows do not touch the crocuses. In that of a friend, some miles off, they attack the yellow ones

exclusively. I address you chiefly to report a fact related to me by the vicar of a neighbouring parish, whose garden is infested with mice. He tells me that for some time he thought he could not grow crocuses at all, as the mice destroyed the corms, discovering and digging down to them, even when there was no trace of the plants on the surface. At last he found that they did not attack the purple crocus, and on his planting the edge of a long border, with alternate clumps of yellow and purple crocuses, the mice almost entirely destroyed all the clumps of yellow, but left the purple untouched. Possibly the purple plant possesses some acid or bitter taste, rendering it nauseous to animals—the corms to mice, the flowers to sparrows and other birds.

Newton-le-Willows, May 4

THOMAS COMBER

Hog-Wallows and Prairie Mounds

IF Mr. Williams is right, and the "hog-wallows" are simply American cousins of our "eshars" or "kames," is it not reasonable to credit that "atmospheric erosion" to which Prof. Le Conte attributes the formation of the former with a much more important influence upon the shapes of the latter than British geologists generally seem disposed to accord to it? It is very difficult to conceive that mounds of loose sand and gravel, whether in valleys or on plains, should have retained the impress of the glacier or the iceberg throughout the vast time that must have elapsed since these phenomena entirely disappeared. And if it be conceded that these mounds have been modified in any degree by subaerial denudation, it will be found difficult to limit the extent to which they are indebted to it for their present forms, or indeed to deny that it alone may have shaped them.

Newport, Fife, May 7

JAS. DURHAM

A "Golden Bough"

IN the gardens of New College, Oxford, there is a fine avenue of horse-chestnut trees, most of which have had some of their lower limbs lopped off, followed by the usual crop of abundant smaller shoots around the original bough. In one tree, however, with respect to one severed branch, these resultant shoots bear, year after year, not green, but pale yellow leaves, the summer through—

"Primo avulsio non deficit alter
aureus, et simili frondescit virga metallo."

It would be interesting to know of other instances of such a veritable "golden bough," and whether any explanation can be given of chlorophyll so remarkably failing to develop its blue-green constituent under no obviously peculiar circumstances. It seems a strange anomaly to find an apparent case of host and saprophyte in one.

HENRY T. WHARTON

SPONTANEOUS GENERATION

ON Friday evening last the Rev. W. H. Dallinger made an important communication to the members of the Royal Institution on "Recent Researches into the Origin and Development of Minute and Lowly Life-forms; with a Glance at the Bearing of these on the Origin of Bacteria." Biological Science to-day presents us with a magnificent generalisation; and that which lies within it and forms the fibre of its fabric, is the establishment of a continuity—an unbroken chain of unity—running from the base to the apex of the entire organic series. But does this imposing continuity find its terminus on the fringe and border of the organic series, and for ever pause there? or, can we see it pushing its way down and onward into the unorganised and the not-living, until all nature is an unbroken sequence and a continuous whole? That such a sublime continuity may be philosophically hypothesized is to be believed. But that data have been presented to us demonstrating how and by what path the inorganic passes to the vital, the living into the not-living, may be denied. The properties of living matter distinguish it absolutely from all other kinds of things, and the facts to-day in the hands of the biologist furnish us with no link between the living and the not-living. This is an inference which has been fiercely disputed.

But what are the nature of the proofs relied upon to establish the "spontaneous" or not living origin of living things? They were chiefly thermal experiments upon the lowest septic organisms, without an attempt to discover what was their life history, and whether they propagated by germs or not. It was argued that the adult organisms being killed at a given temperature much below the boiling point of water, if an infusion were boiled with every possible precaution, and whilst boiling her-